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THE PROBLEM OF JUSTIFYING INDUCTIVE INFERENCE

I WISH to begin with a matter about which, to quote Russell, "none of us, in fact, feel the slightest doubt."[1] I then propose to discuss doubts of that curious academic kind with which philosophers have challenged common sense, with a view to seeing through them clearly enough to find the way back to the common-sense, non-sceptical position. It is not intellectually satisfactory to indulge in the ordinary man's reaction of brushing aside these doubts as trivial or unreal. But it may be possible to cope with them without having to relinquish the common-sense position.

Consider the following quotation from Russell's *Problems of Philosophy:*

> We are all convinced that the sun will rise tomorrow. Why? Is this belief a mere blind outcome of past experience, or can it be justified as a reasonable belief? . . . If we are asked why we believe [this], we shall naturally answer, "Because it always has risen every day." We have a firm belief that it will rise in the future, because it has risen in the past.[2] . . . The problem we have to discuss is whether there is any reason for believing in what is called "the uniformity of nature."[3] . . . Do *any* number of cases of a law being fulfilled in the past afford evidence that it will be fulfilled in the future?[4] . . . When two things have been found to be often associated, and no instance is known of the one occurring without the other, does the occurrence of one of the two, in a fresh instance, give any good ground for expecting the other?[5] . . . If not, it becomes plain that we have no ground whatever for expecting the sun to rise tomorrow, or for expecting the bread we shall eat at our next meal not to poison us.[4]

Russell makes clear that in raising these questions he is not suggesting we seek for a proof that these expectations "*must* be fulfilled, but only for some reason in favour of the view that they are *likely* to be fulfilled."[4] In other words, he is asking for some justification of the inference that the sun is *likely* to rise tomorrow from the fact that it has done so, not of the sun's being *certain* to rise. The task of justifying an inference that something is likely is a more modest one than that of justifying the inference that it is certain. But if both inferences are felt to be in genuine

[1] *Problems of Philosophy*, p. 94.
[2] Pp. 94–95.
[3] Pp. 98–99.
[4] P. 96.
[5] P. 101.

need of justification, then the doubt directed against the weaker inference must be more drastic than that directed against the stronger. Obviously the doubt whether the repetition of concomitances or sequences of events makes a similar concomitance or sequence *probable* is of a more radical kind than the doubt whether it makes this certain. One might feel hesitant, for example, about claiming it was certain that aspirin would stop the present headache on the grounds that it had always provided relief, and not feel at all hesitant about claiming that these grounds made it at least probable.

Such assurance, however, does not for long preserve immunity against the progressive infectiousness of doubt. It is part of the natural history of doubt to spread. Hume raised doubts concerning our certainty of any inductive conclusion p. These can be, and in fact have been, extended to conclusions of the form "p is probable." The same sort of considerations which historically gave rise to the suspicion that even our most assured inductive conclusions are uncertain give rise equally to uneasiness whether *any* evidence could even make them probable. We see this in the progress of philosophers to the more extreme doubt, that brought against the legitimacy, in general, of conclusions of the form "p is probable." In this paper I shall be concerned with doubts of this kind.

It will be useful here briefly to indicate the sources of these in doubts originally raised as to the legitimacy of any inductive conclusion of the form "p." The main sources are two in number: (1) the doubt whether any number of cases of a conjunction of characteristics could make certain any proposition about its continued occurrence; (2) the doubt, even though some number could make it certain, whether one could ever know the number of cases had was sufficient. The first, stated explicitly by Hume, eventuated in the claim that evidence in the form of observations of which we are perfectly certain leaves the inductive conclusion uncertain, the reason being that they are not sufficient for its *deduction*. The fact that two characteristics ϕ and ψ have always been associated is not sufficient to deduce that they always are nor even that they will be in the next instance. To quote Hume:

> There can be no demonstrative arguments to prove, that those instances, of which we have had no experience, resemble those, of which we have had experience. We can at least conceive of a change in the course of nature; which sufficiently proves, that such a change is not absolutely impossible.[6] . . . We have no reason to draw any inference concerning any object beyond those of which we have had experience.[7]

[6] Selections, Scribner's edition, p. 35.

[7] *Ibid.*, p. 43.

These comments apply equally to inferences that something is probable and are explicitly extended to them by Broad when he says, "Now it is quite certain that, if my *only* premise is, 'I have observed N ϕ's, and 100% of them are ψ,' there is no valid form of argument by which I can either prove or render probable the conjecture that 100% of the ϕ's in nature are ψ." [8] Thus the doubt whether any number of cases are adequate evidence for the truth of a conclusion p has grown into a doubt whether they are ever adequate even to render it probable.

And even though some number of cases could make it probable, we can still be uncertain that it is probable, since our source of uncertainty shifts to a doubt very similar to (2) above: the doubt whether any particular number of cases observed is sufficient. If one can doubt whether N cases make p certain, one can obviously doubt whether they make it probable. For example, do five cases of illness after sweet wine make it probable that the next instance of wine drinking will have such a sequel? Or does one need more cases to assure this? Suppose ten more cases were not so followed. Then we should be inclined to say our first five cases were insufficient to infer a probability. But if after our first five, fifty more cases were so followed, we should not know exactly what to say about our previous limited evidence. And no matter what number we mentioned, a question could be raised to throw the inference in doubt. Can we ever know that our evidence is sufficient to warrant "it is probable that p"?

One further uncertainty about the conclusions of inductive arguments, which infects those of ordinary deductions as well, I wish to mention briefly but without elaboration, since it can be interpreted as an uncertainty about a special sort of inductive inference and its special features are not relevant in this paper. This uncertainty concerns, on the face of it, not the validity of inferring a conclusion from premises, but the premises themselves. According to Reichenbach, no premises about physical objects or events could be certain, and hence nothing inferred from them could be. Only propositions of logic or about one's immediate experience are beyond doubt true. It is never absolutely certain, for example, that there is a die before me when I see it, feel it, bet upon it.[9] Sensory observation is subject to all sorts of error, and for that matter we may not even be sure that what our senses reveal is other than the material of dreams or hallucinations. Error lies, not in any judgment recording one's immediate experience, but in the in-

[8] "Mechanical and Teleological Causation," *Proceedings of the Aristotelian Society*, Supplementary Volume XIV, p. 89.

[9] *Experience and Prediction*, p. 331.

ference one makes from it to the existence of a counterpart in the real world. Thus the evidential premise of an ordinary induction is in the position of an inductive conclusion from immediate experience. Any doubt about it will therefore be but a special case of doubt about inductive conclusions in general. Clearly Broad's sceptical doubt could be rephrased, as follows, so as to apply to inferences from immediate experience: Can one's immediate experiences, any number of them, ever make an evidential premise probable? This question has special features which give it an even more academic character than the similar question about inductions made from premises about physical objects and events. In fact one would not in ordinary circumstances raise the question whether it was *ever* certain a die was before one, let alone the question whether this was probable. The first kind of question I tried to deal with in my paper in *The Philosophy of G. E. Moore,*[10] treating it along lines developed by John Wisdom and Morris Lazerowitz. In this paper I shall ignore the special features of scepticism with respect to the senses and confine consideration to the general question: Given that we *know* premises which we take as evidence, can they or *any conceivable* premises make our conclusion probable?

This is but a variation on the old problem, "Can inductive inference be justified?" It is not, however, my intention here to attempt a solution of this problem in the form of a detailed philosophical theory to the effect that probable inference can, or can not, be justified. The questions I should like to answer are quite different, questions which might properly be characterized as "metaphilosophical": Why is there the philosophical problem of justifying such inferences? Is there any genuine problem? The latter question is of fundamental importance. In practice the necessity of justifying every inference, no matter how assured, never presents itself as a problem. Actuaries of insurance companies, after settling the question whether the statistical evidence warrants their probability judgment, would be shocked at the question whether any statistical information could ever justify any such judgment. In calling attention to such a fact, it is not merely that I want to suggest that inductive inference needs no justification, as I think it does not—for this is simply to contradict those who feel it does—but that I want to see if the philosophical problem will disappear when one sees how those who have attempted to solve it have conceived it. In what follows I shall take it that attempted justifications of inductive inference, even though in fact directed towards securing that the inference of p be legitimate, are equally relevant to securing the legitimacy of the inference "p is

[10] *The Library of Living Philosophers,* Vol. IV.

probable." I have already indicated how doubts about the one can be or have been extended to the other, and I shall therefore treat attempts made so far to eliminate these doubts as directed towards securing a probability rather than a certainty.

As one looks through various attempts at justifying inductive inference in general, say those of Mill, Russell, Broad, it becomes clear what it is supposed justification would be like. This is that we should be able to know that p is probable, given that we know such facts as:

(1) that ϕ and ψ have always been associated

or

(2) that ϕ and ψ have been associated in a certain proportion of cases.

Quite a number of different kinds of inference have premises answering to these two general descriptions. From premises of type (1) we infer such conclusions as:

> It is probable that illness always follows large quantities of drink.
> It is probable that illness will follow drink in the next instance.

And from premises of type (2) such conclusions as:

> It is probable that continued throws of a penny will maintain for heads the observed proportion ½.
> The probability of lipoid pneumonia being fatal in this particular case is 9/10.

In connection with such conclusions philosophers have admitted the force of Hume's criticisms. But then, being convinced that nevertheless they are sometimes justified in drawing inferences of this sort, they have gone on to seek the justification. Attempts they have made indicate that what they seek is some means whereby inductive inferences may be made *logically valid in form*, such that the premises *entail* the conclusion "p is probable." This would satisfy the condition that the conclusion be known once the premises are known.

Mill, Russell, and Broad have all, ostensibly, conceived their task to be that of supplying to the inference a premise to be conjoined with the empirical facts about concomitances, from which the conclusion follows. Mill, for example, writes: ". . . every induction is a syllogism with the major premise suppressed; or (as I prefer expressing it) every induction may be thrown into the form of a syllogism by supplying a major premise."[11] What is to be supplied, he says, "is an assumption involved in every case of induction," and this is "our warrant for all inferences from experi-

[11] *Logic*, Bk. III, Ch. 3, p. 224.

ence."[12] Russell holds that "All arguments which, on the basis of experience, argue as to the future or the unexperienced parts of the past or present assume the inductive principle. If the principle is unsound, we have no reason to expect the sun to rise to-morrow, to expect bread to be more nourishing than a stone, or to expect that if we throw ourselves off the roof we shall fall. All our conduct is based upon associations which have worked in the past, and which we therefore regard as likely to work in the future; and this likelihood is dependent for its validity upon the inductive principle."[13] Broad, although he is unable to say precisely what premise conjoined with "I have observed N ϕ's and 100% of them are ψ" would justify him in conjecturing that 100% of the ϕ's in nature are ψ, is in agreement that there must be some premise which does this.

The premise which Russell adjoins to the observational evidence, and which as regards unexamined cases he says "alone can justify any inference from what has been examined to what has not yet been examined,"[14] is as follows:

"(*a*) The greater the number of cases in which a thing of the sort *A* has been found associated with a thing of the sort *B*, the more probable it is (if no cases of failure of association are known) that *A* is always associated with *B*;

"(*b*) Under the same circumstances a sufficient number of cases of the association of *A* with *B* will make it nearly certain that *A* is always associated with *B*."[15]

This is the so-called "inductive principle," as it applies to the probability of a general law. As I understand Russell, he takes this principle to play the rôle of a premise in every induction we make about universal concomitances. In order to exhibit this I set out the following inference, in which I make explicit every premise supposedly used in arriving at the conclusion "*p* is probable." For this purpose I need only part (*b*) of the inductive principle, and in particular a weakened form of it which I shall designate as (α):

(α) Under similar circumstances a sufficient number of cases of *B* accompanying *A* will make it probable that *B* always accompanies *A*.

(β) Under similar circumstances a sufficient number of cases of dry leaves burning when lit will make it probable that they always burn when lit.

(γ) In the past dry leaves when lit have burned a hundred times without exception.

(δ) A hundred cases with no exceptions are sufficient to make it probable that dry leaves when lit always burn.

Therefore, it is probable that dry leaves when lit always burn.

[12] *Ibid.*, p. 223.

[13] *Problems of Philosophy*, pp. 106–107.

[14] *Ibid.*, p. 106.

[15] *Ibid.*, p. 104.

In practice when we infer such a conclusion as this we pass directly to it from (γ). But it will be admitted that (δ), to the effect that we have enough cases to render our conclusion probable, is implicit in this passage. Russell seemingly wants to hold that without (β)'s being true we could not assert (δ). That is, unless there is such a thing as a number of similar cases being sufficient to make it probable that dry leaves burn, no number mentioned would tend in the slightest to do this. (α), as we see, is simply the *form* of this general premise (β), and we must have it if we are ever to have a general premise for any other argument than this one.

As this example indicates, I have taken it that according to Russell finding a justification for an inductive inference consists in finding the premises necessary to make it logically valid, that is, in supplying enough premises to throw it into the form of a deduction. Among these are the inductive principle, and its special instance (β). It will be recalled that those who suppose inductive inference requires justification demand it shall be such that one can *know* the conclusion "p is probable." A simple step to securing this is to exhibit it as the conclusion of premises which are such that if they are certain the conclusion is also. Any deduction satisfies this condition. But this is not sufficient. If the conclusion is to be known, then the premises must be also. And, curiously enough, this according to Russell is precisely what is impossible. The inductive principle, premise (α), he says is "incapable of being proved by an appeal to experience"; "we can never use experience to prove the inductive principle without begging the question."[16] Hence we must simply accept it "or forgo all justification of our expectations about the future."[17] Now it is clear that if a particular inductive conclusion is in doubt *only because* it is doubtful whether any number of cases renders any proposition probable, then one does not justify the conclusion by *assuming* that they do. Nothing is gained by using as a premise what is in doubt. For if one is to establish the conclusion one must know the premises are true. Expressing an inductive argument in the form of a deductive one by supplying the requisite premises thus provides a spurious kind of comfort.

Further, a closer examination of the sequence of propositions, (α), (β), (γ), (δ), makes it very puzzling what function Russell expected (α), or (β), to serve in establishing the conclusion "p is probable." For this conclusion follows from (γ) and (δ) without the help of anything further. In addition (α), and its special case (β), appear to be singularly empty tautologies. (β), for example,

[16] *Problems of Philosophy*, p. 106.

[17] *Ibid.*, p. 106.

states that a sufficient number of cases of dry leaves burning when lit will make it probable they always do. The natural question is "sufficient for what?" and the natural answer is given in the following paraphrase of (β):

If the number of cases of dry leaves burning when lit is sufficient to *make it probable* that they always burn, then it is probable that they do.

(α) permits of a similar paraphrase. It is clear that the addition of these necessary propositions to (γ) and (δ) will in no way help to deduce the conclusion. In fact as the use of truth-tables shows, the conjunction of a necessary proposition p with any contingent proposition q, or set of such, will have no more deductive fertility than the contingent proposition alone: $p.q. \equiv .q$. So giving our inductive argument a deductive form can not in this case be described as consisting in the addition of premises ((α) and (β)) without which the conclusion could not be inferred. On the basis of this account, the relevance, let alone the importance, of the inductive principle in a particular inference is very doubtful.

I think some account of this principle remains to be given.[18] I have pointed out that neither (α) nor (β) is a proposition with the help of which the conclusion is deduced. That is, their rôle is not like that of the general premise of a syllogism, *e.g.*, "All men are mortal," without which the conclusion, "Socrates is mortal," can not be inferred. Suppose now for purposes of comparison with our inductive inference we conjoin to the familiar syllogism the proposition

(θ) If all men are mortal and Socrates is a man, then Socrates is mortal

and that to this we add

(ω) If all a is b and x is a, then x is b.

The adjunction of (θ) and (ω) to the premises of the syllogism is of no help in deducing "Socrates is mortal"; the premises alone are sufficient for this. (θ) and (ω) are both necessary, and hence could not help establish an empirical conclusion. However, the fact that (θ) is necessary is the same as the fact that "Socrates is mortal" may be deduced from the two given premises. And the fact that (ω) is necessary is what is meant by saying this syllogistic argument is valid in virtue of its form. (θ) and (ω) are not premises, but *rules* according to which deductions are made, (ω) being the general rule of which (θ) is the special instance.

Now there is some analogy between the rôle of (ω) and the rôle of (α), the inductive principle. If we put (α) in the form

[18] What follows by way of analysis of it I owe in large part to discussions with M. Lazerowitz.

If for some specified number N, N cases of association of A and B occur and if these are sufficient, then it is probable that A and B are always associated,

it appears, not as a premise, but as giving the *form* of the inference

(γ) Dry leaves when lit have burned one hundred times without exception.
(δ) One hundred cases are sufficient.
Therefore, it is probable dry leaves always burn when lit.

A similar thing was pointed out by Whiteley in his paper in *Analysis*,[19] where he maintained that the inductive principle expresses the form common to particular inductive arguments. Unless the form is valid, the particular inductive argument can not be. The same, of course, characterizes (ω), the form of the syllogistic argument cited. The difference is that whereas everyone would grant that (ω) is the rule according to which "Socrates is mortal" is validly deduced from its premises, it is not as clear, and philosophers such as Broad are ready to challenge, whether (α) is the rule according to which inductive conclusions are inferred. For one thing, the hypothesis of (α), "If for some number N, N cases . . . are sufficient," implies the possibility of *some* number of cases being sufficient, by itself, to render a proposition probable. And this is what Broad denies.

Of course (α), as stated, is undeniable: If some number N of cases occur and are sufficient to make p probable, then p is probable. It can, however, be restated so as to free it of its trivial appearance. In the following form (α) does not look as certainly, nor as trivially, tautologous:

***A sufficient number* of cases of association of A and B make it probable A and B are always associated.**

"A sufficient number" seems to denote any of a sequence of numbers having a lower limit, so that (α) might be replaced by:

***Some number* of cases of association of A and B makes it probable they are always associated.**

Here we have the proposition which Broad claims is certainly false, since according to him it is insufficient for making any proposition probable *merely* that there should be N cases of association without exception. Yet one feels inclined to agree with Lewy that in accordance with the English usage of the word "probable" this is a necessary proposition, so that to assert the observational premises of certain inductions and deny that the conclusion is probable is a self-contradiction.[20] "Some number of cases makes p probable" looks to be strictly analogous to the necessary proposition, "Some

[19] "On the Justification of Induction," *Analysis*, Vol. 7 (1940), p. 69.
[20] "The Justification of Induction," *Analysis*, Vol. 6 (1939), pp. 89–90.

number multiplied by 2 equals 14." These two propositions can be instructively compared. The function on which the latter is constructed, namely, "$x \times 2 = 14$," is such that it has a necessarily true value, "$7 \times 2 = 14$." Is it similarly the case for the function "x cases make p probable"? If it is, and if in the inductive inference we have used as an example, 100 is a value rendering it tautologous, then our premise (δ), that one hundred cases are sufficient to make it probable that dry leaves always burn when lit, plays no rôle in deducing the conclusion. This can be deduced from (γ) alone, that dry leaves when lit have been found to burn one hundred times without exception. "One hundred cases make it probable that dry leaves always burn" would not be needed for passing from (γ) to the conclusion any more than "All squares are four-sided" is needed in passing from "This is a square" to "This is four-sided." A necessary proposition, to reiterate what was said before, can never be a *premise* with the help of which an empirical conclusion is deduced. But of course if a given proposition of the form "x cases make p probable" is necessary, then the conclusion "p is probable" follows necessarily from the empirical evidence. And our necessary proposition functions as a rule according to which we deduce it.

Now it is quite obvious that whatever the analogy between "Some number of cases makes p probable" and "Some number multiplied by 2 equals 14," the lack of analogy is equally obvious, and it is this which vitiates Lewy's contention against Broad. The function "$x \times 2 = 14$" has but one tautologous value, whereas "x cases make p probable," if it has any tautologous values, will have many—all those from some number N on. And the difficulty is that we do not know at precisely what point the function becomes tautologous, if it does so. This is merely another way of putting the uncertainty expressed in the question, "How can one know the number of observed cases is sufficient?" One knows by inspection precisely when one has found the value which makes "$x \times 2 = 14$" necessary. But one does not know by inspection whether the empirical evidence entails "it is probable that p."

It is doubtless this fact which is responsible for the feeling that inductive inference needs justifying. Usually this justification has been conceived as a matter of supplying, not a validating rule, but some general empirical premise. At least there is some problem about induction which supplying this is supposed to solve. This problem is, I believe, not "How can any number of cases render anything probable?" but some other problem which, even though solved, would leave the question about cases unanswered. I shall try to support this claim in what follows, for this is not the

accepted view about what is gained by supplying a general premise about the constitution of nature. Broad, for example, holds that in order to know a conclusion of the form "*p* is probable" it is necessary that we know some premise of this sort. In general his view is that inductive arguments all involve a premise to the effect that there is in nature constant causal determination and something like "loading" in favor of one alternative rather than another, and that in order for this to be the case nature must answer to the principle of limited variety.[21] Very roughly this principle is that nature is not infinitely complex—that every object and event analyzes into a limited number of elements and hence that a limited amount of variety is possible from combinations of these elements. Again very roughly, what would be secured if the general empirical premise were true is that nature obeys simple causal laws.

Now my thesis is that this concern that nature obey simple causal laws is a concern that it be possible for us to make inductive inferences at all, not that it be possible to *know*, once the conditions for making an inference are satisfied, that any given probable conclusion is validly inferred. That is, there are two questions, "What would justify us in making inductive inferences, *i.e.*, what would make it possible to do this?" and "What would justify, in the sense of 'validate,' the conclusion '*p* is probable'?" Broad has, I think, answered the first question while supposing himself to have answered the second. He makes a premise about nature a requisite for deducing the conclusion, whence we could not know "*p* is probable" without knowing this premise. I want to consider briefly what the consequences are if the premise about nature is false. I am not certain, but I believe that if nature is not of such-and-such a constitution inductive inference would not be possible. And in this case we should have no problem of validating an inductive conclusion. The problem as to what validates an inductive conclusion can arise only if it is possible for inductive inferences to be made.

From a number of statements about nature which have been cited as premises of inductive inference I shall select a few typical ones and see what their falsity entails. Usually, to put it vaguely, it has been held that nature must be uniform, that the law of universal causation must operate. Now, it is not sufficient that the law that every event has a cause and similar causes have similar effects hold. As Russell said, "it is not the reign of law but the reign of *simple* laws which is required."[22] But now for what is it not

[21] "The Principles of Problematic Induction," *Proceedings of the Aristotelian Society*, Vol. 28, p. 45.

[22] *Analysis of Matter*, p. 232.

sufficient, for what is the reign of *simple* laws required? Both Venn and Keynes have provided an answer which supports my contention that the premise about nature which is being sought is one which assures the *possibility* of making inductions. Keynes says [23] that if every configuration of the universe were subject to a separate and independent law and if every difference in bodies led to their obeying different laws—in other words, if it were false that nature is under the reign of simple laws—it would still be the case that nature was uniform and causation sovereign; but prediction would be impossible. If inductions are to be made at all, nature must not be like this. Nor must it be such that though every event is causally determined no causal law has more than one instance in the history of the universe. As Venn points out,[24] induction could not take place in such a world either. Here the reason we should not be justified in inferring "p is probable" would simply be that we could not start to make any inference. We should not have any observational premise about repeated concomitances.

The point is clearer if we consider the consequences of falsifying the premise Mill cited as being a warrant for every induction. According to Mill it is a necessary condition for proving any inductive conclusion that nature be uniform. By the uniformity of nature he meant that there are in nature such things as parallel cases —that it is sometimes the case that what is true of one instance of ϕ is true of all instances. In other words, some generalizations hold in nature, or there are uniformities in nature. Let us suppose there are no uniformities in nature, simple or otherwise. In this event two situations are possible: (1) that no characteristic occurs with any other in accordance with any law—that is, that there are no constant conjunctions of any characteristics, but that each occurs with absolutely any other; (2) that there are some repeated concomitances and sequences but that their repetition is not universal. In the first case we clearly have a suspension of the principle of limited variety. In such a world the amount of possible variety would be unlimited. In fact there could not even be what Broad has characterized as "natural kinds"—groupings of characters which constitute such recurrent entities as humans, horses, trees, etc. Were a malignant demon to scramble up such a world, it would be one in which no inductions could be made. For one thing, there would not be human beings to make them. In fact it is doubtful whether what is being described here is even a conceivable situation. As Isaiah Berlin points out,[25] in the extreme case

[23] *A Treatise on Probability*, p. 249.

[24] *Empirical Logic*, pp. 96–97.

[25] "Induction and Hypothesis," *Proceedings of the Aristotelian Society*, Supplementary Volume XVI, p. 99.

where no character ever occurred more than once, we should have no general terms to refer to it, and such a chaos could not even be talked about let alone inferred from.

However, the case is different if there are some repetitions of sequences or characters, even for relatively short periods of time. Suppose that for some centuries there has been that constant conjunction of characters which constitutes mankind, and that other conjunctions of characters are repeated for a couple of generations. Is inductive inference possible? First of all, if we *knew* it to be false that there were any universal conjunctions we should of course never say it was probable that ϕ and ψ were always associated. However, if it were false that there were uniformities, but because there were some repetitions and recurrences it was not *known* to be false, we could and would still make probable inferences. We should have the same kind of evidence which we now have, the sort expressed in premise (γ), namely, in one hundred cases without exception dry leaves when lit have been found to burn. What we do in establishing the probability of a special uniformity is to make an inference from observed repetitions. Of course if our ancestors told us that things had been different in their day we should be uncertain whether it was probable the next instance of ϕ would be ψ or that all future instances of ϕ would be ψ. And if ϕ and ψ were never observed in conjunction again we should not say it was probable ϕ would be associated with ψ in the next instance. In fact after a time we should most likely say it was improbable.

Exactly how much repetition there must be before we draw probable inferences I do not know, but certainly in a world where there was very little we could not draw them. For we should lack the observational premise, *e.g.*, "One thousand cases have been found without exception . . . etc." And we should need this for inference regardless of what other premise might be needed. The point I am trying to make here is that the question as to what nature must be like before we are justified in making any inductions at all, *i.e.*, what it must be like to have proper observational premises, is different from the question whether, given the observational premises, we are justified in drawing a conclusion from them.

I want to return now to this latter question. The fact that philosophers feel that there is a problem here shows some sort of dissatisfaction. To say what this dissatisfaction is, when we in fact do not for a moment feel uncertain that, for example, the sun will rise again and the springtime come, is to answer *our* question, Why is there the philosophical problem of induction? It seems to me this is a question which ought to be asked. For this need of

justifying inductive conclusions never is felt in any ordinary circumstances. Our behavior shows this: We are willing to bet that 1, 2, 3, or 4 will come up on a fair die, as against 6. We never become fearful that the laws of nature will be suspended, so that we hesitate to plant a garden on the grounds that the sun may never shine again. And yet, despite this, we feel challenged by the question, "Are you warranted in supposing it probable the sun will shine again?" as we are by the question, "Are you warranted in supposing it probable that virus *A* is responsible for the illness of all these guinea pigs?"

Certain linguistic analogies between questions and doubts philosophers express and questions and doubts of the scientist or ordinary man are, I think, responsible for our feeling the challenge. Either the questions and doubts give vent to some dissatisfaction or they positively create it. It is clear that the request that a scientist justify a particular inductive conclusion and the request that inductive conclusions in general be justified bear at least a verbal analogy to each other. And the doubt expressed in "She loved you today and yesterday, but have you any good reason to suppose that she will love you tomorrow?" is analogous in the same way to the seeming doubt expressed in "Yesterday, and for many years before that, things which were dropped fell; but have you any good reason to suppose the law of gravitation will continue to operate tomorrow?"

Further, there is close enough analogy between inductive inferences and deductive ones that questions whether an inference is warranted seem to be such as could be settled. In a deduction it is always possible, even though sometimes difficult, to determine whether a conclusion is validly drawn. And when it is, it is a misuse of language, *i.e.*, self-contradictory, to assert the premises and deny the conclusion. The linguistic pattern of inductive arguments is very similar to that of deductive ones, and there are some cases in which it definitely seems a misuse of language to assert that a conjunction has been repeatedly observed and then to deny the probability of its occurring in a fresh instance. One would feel that a person who said "I have known thousands of cases, and no exceptions, of people being burnt when they touched a hot stove, but it is not probable that I shall be burnt the next time I do it," or "The penny has come up heads a million times straight, and never tails, but it is not probable the penny is unfair," was somehow misusing language—that he didn't know the use of the word "probable," and was saying something self-contradictory.

Now despite all these analogies there are equally obvious lacks of analogy. To ignore these is to fail to see that questions and

doubts which the sceptic has about induction are but pantomimes of questions and doubts which are genuine. For example, to ask of a scientist whether his evidence is sufficient to warrant a particular inductive conclusion is to ask a useful question to which certain procedures he commonly engages in are relevant by way of answer. Should he be made unsure by the question, he will reconsider his evidence, and, if satisfied, will reiterate his probability judgment, and, if not, will experiment further. By further tests he could, for example, increase or decrease the probability that a particular virus induced illness in his guinea pigs, or he might even disestablish the probability judgment previously arrived at. But now the question, "Are any empirical facts evidence for any conclusion '*p* is probable'?" is not one to which any testing procedure is relevant. Nor is it useful to ask it. For it does not create a doubt which in any way affects our behavior.[26] We don't become uncertain, in virtue of *this* question, of the conclusion of which we were certain.

However, the sceptic's comment, "You must admit that the fact that a law has operated in the past is thin reason for expecting it to continue," manages somehow to be convincing. The main reason for this I believe to be the following: Despite the fact that some inductive arguments closely parallel deductive ones, so very many do not. The sceptic's emphasis on the lack of parallel makes us intellectually uncomfortable even with regard to those inferences we previously felt sure of. For example, we all feel that there would be something peculiar in saying "I have a million favorable cases and no unfavorable ones, but still *p* is not probable." This seems to be a misuse of language. But even here we are not as sure there is an entailment relation between the observational premise and the conclusion "*p* is probable" as we are in the case of deductions which are valid in virtue of their form. This uncertainty I believe arises from the fact that we can think of innumerable cases where one doesn't know whether to say the inductive conclusion follows or not. Suppose that one hundred cases of my drinking coffee at night were followed by sleeplessness, and that this did entail the probability of my spending a sleepless night next time, would fifty cases entail it, or twenty-five, or ten? There is no way whatever of deciding this. And the indecision about the special inference spreads to all inference, despite the obvious lack of analogy, already cited, between the genuine question, "Have I suf-

[26] See "Moore's Paradox," by M. Lazerowitz, in *The Philosophy of G. E. Moore*, for a discussion of the academic character of a similar philosophical doubt; also "Philosophical Perplexity," by John Wisdom, *Proceedings of the Aristotelian Society*, Vol. XXXVII, pp. 78–79.

ficient evidence to warrant this inference?" and the question, "Have I ever sufficient evidence to warrant any inference?"

It is one of the features of a philosophic problem that we feel puzzlement and yet not of a kind which in any way alters our behavior. Here we have an instance. In practice we do not doubt that we are in general justified in making inductive inferences—though we all grant that sometimes we are not; and yet certain considerations produce an academic dissatisfaction with inductive inference in general which makes us try to justify it. This justification is conceived as a matter of finding a means whereby, being certain of the observational premise, we can be certain that p is probable. I want to say that the remedy for the dissatisfaction we are made to feel about inductive inference is misconceived. If this can be shown then it will be clear why there is the philosophical problem about inductive inference. I shall begin by tracing the dissatisfaction and the attempt to allay it.[27]

Let us begin by comparing ourselves with an imaginary tribe of beings who are unwilling to call any proposition probable unless supported by a million cases. Such beings would doubtless complain over seldom being able to establish any probability. But would their situation be like our own? Consider the following inference. From this we shall see how dissimilar our situation is, and why the dissatisfaction, which made Broad say no number of cases by itself could make p probable:

> Three cases of drinking coffee at night were followed by sleeplessness. Therefore, it is probable that drinking coffee at night is always so accompanied.

Now suppose coffee was drunk at night fifty more times without any unpleasant sequel. This would make us say that our first three cases were insufficient to establish the probability of coffee's causing sleeplessness, that is, that it is false that the observational premise implies the conclusion. It might then be argued that if three cases wouldn't necessitate p's probability, would seven, or eight, or ten? Can one case ever make the difference between the implication's being false and its being necessary? And even though we agreed with Lewy that some number of cases would make the implication necessary, and even agreed on some particular numbers which did this, we should soon find ourselves in the position of being unable to decide, as we diminished the number of cases, the precise point at which the implication became non-necessary. It is an impressive fact that on substituting consecutive values in the function "x cases makes p probable" we derive a numerically ordered

[27] In this I am indebted to discussions I have had with M. Lazerowitz.

sequence of propositions with regard to which there is no reason for saying any one is necessary and its immediate predecessor is not. Apparently Broad is so impressed by this fact that he is willing to deny that any of them is necessary, which is to deny that any number of cases make p probable. We are in a much worse position, seemingly, than the people who must find a million supporting cases before they can infer a probability. We do not know exactly how many cases we need. The sceptic makes us feel that we should. In these circumstances it is natural to conceive our task as *finding* either a premise to conjoin with the evidence from which "p is probable" would follow, or a rule which inductive inference must conform to if valid. I have tried to argue that what is being sought is a rule or rules, since a premise about nature secures something else.

Now what would such rules do? Clearly what they do in deductions—provide criteria for determining whether the arguments are correct. This means providing exact criteria for the use of the word "probable," *criteria fixed by the number of cases,* such that from a definite number of cases it can be known that "p is probable" follows. If such were found we should know which values of x in "x cases make p probable" yielded something necessary. Whenever the premise involved one of these numerical values we should know it entailed the conclusion just as we know that "this is square" entails "this is four-sided" because we know "all squares are four-sided" is necessary.

Finding such rules would certainly provide us criteria for correct inference. But now where should one look for them? Let us consider what Aristotle did in formulating rules for valid deduction. He examined arguments accepted as correct and derived the rules from them. What is the case with inductive inference? Here, even in those instances when no one doubts that the conclusion is warranted, inspection doesn't yield the rule it conforms to. Now a rule is merely the abstract form of a class of inferences already accepted as correct. But if no rule is found in the inferences themselves, is there a rule? Aren't these our only source for the discovery of rules? If examination yields no rules then our inability to find them is simply due to there not being any. This is, I think, what is not seen by those who demand some justification for induction. They conceive justification as consisting in finding what is hidden, and that if one fails no inference can be known to be valid.

The dissatisfaction with inductive inference which prompts the quest for a hidden principle which justifies it arises from the following commonplace: that instead of certainty that "p is probable"

follows from the empirical facts, in case after case there is indecision. One does not know whether the evidence is sufficient, nor how to test whether the evidence is sufficient. If we look at the English usage of the word "probable," we see that "Some number of cases makes p probable" does give the appropriate context for probability statements; that is, it is a sort of rule for the use of the word "probable." But it does not tell us specifically with *what number* of cases we may use the word. It provides no exact rule, no rule that specifies the precise minimal number of cases from which "it is probable that p" *follows*. The complaint is that an inexact rule is no better than no rule at all.

It thus turns out that the apparent dissatisfaction with not being able to find rules is in reality a dissatisfaction with the linguistic fact that there are no exact rules. The philosopher's complaint is quite unlike that of the people who find it difficult to establish a probability because they require for this a million cases. Their language contains a definite rule for saying when a proposition is probable. The philosopher's complaint is that ours does not. We do not know at what precise point a given proposition becomes probable, for in our language this has not been decided. That is, we have no fixed criteria in our language for saying whether an inductive argument is correct. The pair "probable," "not probable" are vague in much the same way "rich" and "poor" are. When a man has only a penny to his name and when he has a million dollars we are certain of the application of the terms "rich" and "poor." But for a whole set of values in between we do not know what to call him. And it is this fact that makes it tempting to argue that no man could be rich since the difference between being rich, poor, or any state in between, could not be in the having or lacking one penny. It seems that there should be some sharp dividing line between these states, some point at which it is proper to apply the one term and not the other. Similarly with the term "probable." For a whole middle range of values of x in "If x cases . . . occur, then p is probable" we do not know whether it follows that p is probable or not. To know this requires knowing which values when substituted in the function yield something necessary, and this has not been decided. Nevertheless it seems that there should be some point at which the function becomes necessary, before which it is not.

There *should* be such a point—that expresses exactly the dissatisfaction about inductive inference. The complaint that we do not in given cases know whether we can validly infer that p is probable appeared to be the complaint that we had *found* no rules to which valid inductive inference conformed. The remedy of this situ-

ation would be had when these were found. But this puts the philosopher's dissatisfaction in a misleading way: as though it were over the inability to find rules rather than over the lack of them. The fact is that nothing is hidden. If to justify induction we must look for rules according to which one correctly deduces "*p* is probable," we are looking for what does not exist. But if the feeling that induction needs justifying is merely the feeling that there *should be* rules of this sort then the means of remedying the lack is not to be described as finding them, but as making them. To ask that the criteria for the word "probable" be more exact than they are, that there be a sharp line between correct and incorrect inductions as there is amongst deductions—this is a way of urging that exact rules be made up in such a manner that in being certain of the observational statement we can be certain of "probably *p*." This is, of course, to urge a modification of our language. And to do this is to express a dissatisfaction with it. Here I think we have our answer to the question, "Why is there a problem of justifying inductive inference?"

I can not here go into whether this urge to modify the language so as to remedy what appear to be its inadequacies is justified. But I will point out that if we try to eradicate the vagueness of the word "probable" we should most likely want hundreds of rules, and all of them would appear quite arbitrarily formulated. For example, we could make it a rule that fifty or more cases of dry leaves burning when lit made it probable they always would burn, and that twenty-five or more cases of sleeplessness following coffee at night made it probable it always would follow. And so on. We should want a rule for each inference. And in no case would there seem to be any special reason for accepting the rule. Why should twenty-four or forty-nine cases be insufficient—why should an inference from these not be necessary while an inference from twenty-five or fifty cases would be? Undoubtedly if such rules were accepted philosophers would find them the source of a new problem. Their complaint would in these circumstances be against the excessive sharpness of the word "probable" instead of its excessive vagueness. The new dissatisfaction would arise from the *arbitrariness* of the rules rather than the lack of them.

It would seem that the word "probable," like the word "rich," is for the purposes of ordinary life as sharply defined as we want it; and even philosophers have shown no real interest in sharpening the criteria for its application else they would have tried to still their dissatisfaction by the only means open to them, namely, by specifying such criteria. All this indicates that present usage is satisfactory, otherwise we should have what we do not now have,

precise criteria fixed by the number of cases. Once this is admitted, the need to justify probable inference disappears. At this point we have returned to the common-sense position.

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BOOK REVIEW

A History of American Philosophy. HERBERT W. SCHNEIDER. New York: Columbia University Press. 1946. xiv + 646 pp. $4.50.

American Philosophic Addresses, 1700–1900. Edited by JOSEPH L. BLAU. New York: Columbia University Press. 1946. xii + 762 pp. $6.75.

If these companion volumes should succeed in attracting the readers which they deserve, the understanding of the history of American thought would be notably advanced in our generation. For they are not only mature works in their own right, but also remarkably useful tools of scholarship for further research in a field that is still comparatively new. Only those who have labored long with some of the same problems will appreciate fully the sagacity and the thoroughness of Professor Schneider's long-awaited history. As Number 18 of the Columbia Studies in American Culture, it is the distilled wisdom of a program which both anticipated and accelerated the present wave of interest in the subject. It is not a book for beginners. It is a superbly suggestive source-book and manual for teachers, but most students will have to earn the privilege of reading it effectively. Concerning its bibliographies, the work of Dr. Blau, it is sufficient to say that nothing like them has been seen before. They are immeasurably superior to any previous attempts, and constitute a landmark in the study of American philosophy.

Yet at the same time that these books rescue one impressive American thinker after another from oblivion, they raise a host of new and puzzling issues about the comparative stature of those with whom we are already familiar. Professor Schneider properly calls his book *A History,* not *The History,* and is quick to admit that "a truly comprehensive history of American philosophy still remains to be written." The writing of American intellectual history, he adds, "is in a highly experimental stage, and I have no illusion that my outline will endure when my successors get to work" (p. x). Nevertheless he has taken long strides beyond his predecessors, and those who come after him will find their paths much smoother because of his laborious efforts.

It might be said that every word in the title *A History of American Philosophy* raises controversial issues. There is the ancient debate over whether philosophy is to be defined as "the college subject" or whether it may embrace all highly general, inclusive, and self-critical thought. The author fortunately decides in favor of the latter alternative, which enables him to admit freely such academically doubtful figures as Franklin and Barlow, Hawthorne and Melville, Terence V. Powderly and Edwin Arlington Robinson. But there are also those critics who object to any juxtaposition of the adjective "American" and the noun "philosophy" on the ground that it smacks offensively of nationalism in a subject that should always be regarded as nothing less than universal. There is, they say, no French or Liberian or Japanese or American philosophy; there is only philosophy, although it may be cultivated *in* France or Liberia or Japan or the United States. Then there is the additional quibble that this book is not a history of all American philosophy, but merely "A History of Philosophy in What Is Now the United States." It should also be noted, for the reassurance of those who deplore the present rash of "We *versus* They" thought-patterns in the field of international relations, that this book lends no aid or comfort to those who may be seeking "an American philosophy" for "the American century" or for export to the outposts of an American empire. The author finds no "central content, dominant note, or moral lesson" in the traditions he surveys, and judges that "the reader of this story will probably be at least as bewildered as I am in trying to tell what American history teaches us or what American philosophy 'stands for.' . . . Our past is fully as confused as our present" (p. ix).

Professor Schneider, in other words, admits himself the confused historian of a confused people, and firmly resists all temptations to interpret American philosophical history in terms of a single theme or idea. This will not gratify those critics who have strong cravings for unity and order in written history, but it is by all odds the policy most appropriate to the actual situation. The one thread which seems to run through the entire story is that of an optimistic and pluralistic hospitality toward ideas from almost any source, plus a strong tendency to put all notions to the test of practice.

When a new idea comes to us, we project it, semi-consciously, against one historical perspective after another to discover what meanings or significances it may acquire when placed in various contexts. After a series of experiments in interpretation, we discover how and where the new idea can best be used and assimilated. The variety of contexts at our disposal gives us many handles by which to take hold of novelties. But we do not take hold of them

as much as they get hold of us. The many ways in which we resist, distort, adapt, revise new importations is the best evidence that an American tradition lives. [P. ix.]

In deciding the amount of space to be given to any particular subject, Professor Schneider emphasizes the little-known at the expense of the well-known. If there is a defect in the architectural scale of the book, it is in the scant thirty pages allotted to the colonial period, even though the material is drastically and skillfully condensed. The reason for this is undoubtedly the existence of extended treatments by Perry Miller and Ralph Barton Perry, as well as Schneider's own earlier *The Puritan Mind*. Yet it is hard to escape the fact that a history of American philosophy must perforce begin in England, with the complicated theological and political heritages of the groups which happened to emigrate, and to become philosophically articulate, in New England. The author shows clearly how it came about that

the transition from Puritan Platonism to deism and natural religion was easy, gradual, and largely unconscious. For the Puritans were obviously not so dependent on the Biblical revelation of law and covenant as they pretended to be. Their system was from the start more genuinely philosophical than Biblical. [P. 9.]

But for the detailed story of the way in which that system emerged from medieval English Catholicism, socially and economically as well as politically and philosophically, the reader must and should go elsewhere.

Part II tells in six brief chapters the exciting story of the American Enlightenment.

Never in America were philosophical thinking and social action more closely joined. . . . Never was history made more consciously and conscientiously, and seldom since the days of classic Greece has philosophy enjoyed greater opportunity to exercise public responsibility. . . . [Small wonder that today] no American thinker who is more than a professor can refrain from occasional thoughtful wishing for the utility and freedom which philosophy then enjoyed. [Pp. 35–36.]

To those who have been puzzled by the apparently deliberate fusion of philosophical paradoxes embodied in our constitutional system, a careful reading of the section called "The Theory of Freedom" is strongly recommended.

The American Enlightenment was full of glorious promise of a secular humanism to come, but in a sense it failed miserably to consolidate itself institutionally, and was followed by "a thorough and passionate reaction against its ideals and assumptions." Yet there had been an influential and irreversible "invasion of America by Roman law and Greek political philosophy"; and, more impor-

tant still, natural science had won a foothold, even though the freethinkers had been overwhelmed with pious abuse. "In the natural sciences the Enlightenment still lives, but the so-called mental and moral sciences were fated to endure another century of philosophical polemics" (pp. 35, 45, 76).

Up to this point, relatively few unfamiliar faces have entered the story, and it has moved very rapidly, but in Part III, entitled "Nationalism and Democracy," we encounter one "unknown" after another, nearly all of them deserving the attention they receive. Among them are: Daniel Raymond, the Baltimore lawyer who provided "an excellent philosophical orientation" for what we know as "welfare economics" as early as 1820; Richard Hildreth of Deerfield, a radical utilitarian in the 1840's and "America's only Bentham"; Francis Lieber of Germany and South Carolina, with his organic institutional decentralization as the key to liberty; and Elisha Mulford, "more of a scholar than a preacher," who fused religious language and emotion with idealistic democratic nationalism. At the risk of seeming to exaggerate their new-found importance, the author lets each of these neglected prophets speak for himself in copious quotations. He shows a keen insight, not only into the vagaries of the numerous "frontier faiths and communities," but also into the peculiarly American varieties of socialism (Henry George, Bellamy, Gronlund, Powderly) which so continually baffle the European observer.

> It was good fortune rather than good judgment, which gave to the United States its national socialism before it received its Marxism. . . . There was more recognition of class struggle in the politics and theories of James Madison, of Calhoun, and of Webster, before the industrial revolution had reached its height, than in the days of Bellamy, Bryan, Powderly, and Carnegie, when there was a conscious effort to avoid further struggle. The preaching of cooperation had *behind* it the bitter memories of conflict; it was a philosophy of industrial innocence, but of political maturity. These circumstances of American bourgeois socialism require that we interpret it as indigenous and not merely as an extension of European movements. . . . In America such religion was neither an opiate of the people nor a prescientific mythology; it was a carefully constructed mythology for wakening Puritans out of their false sense of security. [Italics his, p. 207.]

It is in Part IV, "Orthodoxy," that "academic scruple and professional etiquette" seem to have required the exhumation of more than one thinker who is unlikely, as the author says, to "suffer further revivification." But Part V, "The Transcendental Temper," soon makes up for any temporary dullness. Among the romantics, Theodore Parker, James Marsh, Caleb Sprague Henry, F. H. Hedge, and that heaven-storming pair from Albany, Herman Melville and Henry James the Elder, receive an amount of atten-

tion which reduces somewhat the conventional importance given to Emerson, Thoreau, and Alcott. It is not surprising that it is in this period that Professor Schneider's account is most fully supplemented by *American Philosophic Addresses,* including two by Emerson, and others by Parker, Channing, Brownson, and James. Part VI on "Evolution and Human Progress" carries the story of American naturalism from John Fiske and Chauncey Wright to a "desperate" quartet: William Graham Sumner, Henry Adams, E. A. Robinson, and George Santayana, with new faces appearing in the persons of Francis E. Abbot, E. D. Cope, and Edmund Montgomery.

Idealism, the subject of Part VII, when it succeeded orthodoxy, "produced a revolution in American higher education and a renaissance of the pursuit of philosophy as a reflective, systematic enterprise." It is hard to see how an idealistic historian could do greater justice to this part of the story than does the author, who has already emphasized the great importance of Hegel in averting an undemocratic turn in the theory of American collectivism. He revives the almost-forgotten Laurens Perseus Hickok and his disciples Seelye and Garman, and tells the story of the western and eastern "schools." Royce, he remarks, "succeeded in being a school all by himself." He finds, "perhaps prematurely, that there are emerging two types of idealism . . . a moral and a natural idealism . . . which may even be regarded as complementary to each other" (pp. 493–494).

The concluding Part VIII, "Radical Empiricism," which winds up as "Empirical Radicalism," tells from intimate knowledge the story of pragmatism from its beginnings to its practical applications by Dewey, Justice Holmes and his associates, the Chicago trinity of Merriam, Lasswell, and T. V. Smith, and Dr. Albert C. Barnes. The closing words are John Dewey's, and are to the effect that need and desire "continually open the way into the unexplored and unattained future." Professor Schneider declares too dogmatically that "Part IX, entitled 'New Realism and New Naturalism,' cannot yet be written . . . the careers of these ideas are still too young to merit biography, and their significance is still veiled in the future. The historian must therefore be content to end his story where his own reflections begin" (p. vii).

In a work teeming with thousands of factual statements, it is amazing that the percentage of damaging errors has been kept so low. In later printings Francis Wayland will attend Union College, not Union Seminary (p. 242); Melville will be born in 1819 and be seventeen when he goes to sea (p. 293); Ungar will be an American critic (p. 299); and the biographer of A. H. Lloyd

will be E. S. Bates, not E. S. Brightman (p. 506). Without extensive quotations it is impossible to convey the wealth of illumination which the author has compressed in brief asides, and the subtle and witty way in which his excerpts point up their writers' excellences or shortcomings. But this the discerning reader will discover for himself.

Dr. Blau, for his part, has tapped a new vein of intellectual history by discovering the philosophic address. The latter is a public oration "speculative in nature . . . a popularized statement of a philosophic outlook as well as a call to a particular action or belief." Excellent as nearly all of them are, they make us realize anew the auditory patience of our forbears, who sat through some very long and exhausting utterances. Six of the twenty-seven selections were addressed to chapters of Phi Beta Kappa, and several more to academic convocations of one sort or another. But only presidential addresses to learned societies would dare to approach their average length in our effete day.

"The Philosophy of American Culture" is the theme of the first group of speeches leading up to Emerson's "The American Scholar," Henry James the Elder's "The Social Significance of Our Institutions," and Wendell Phillips's ringing "The Scholar in a Republic." Science, which is the subject of the second group, does not lend itself so well to oratory until Darwin comes upon the scene, and then the controversy is too often blind and pointless. In the concluding section on "The Philosophy of Religion" it is no surprise to find that the liberals (Bentley, Channing, Emerson, Bushnell, Parker, Abbot, and Munger) predominate over the conservatives (Edwards, Ely, and Park). It is a pity that Dr. Blau was not able to include a group of addresses on political philosophy, since they would have formed a very valuable supplement to Schneider's history. He prefaces each address with a biographical calendar of its author, and an enlightening analysis of its setting and significance.

It is fashionable nowadays to profess to write the history of philosophy in close connection with its political, economic, and cultural backgrounds. But not all who make this profession of intention actually succeed in carrying it out, as Bertrand Russell's recent volume shows. Messrs. Schneider and Blau make only modest and cautious claims, but their joint accomplishments speak louder than the loudest of publishers' blurbs. Their spades have been busy in neglected corners of a field close to home, and their first harvest should be a shining example to their compatriots.

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BOOK NOTES

Realists and Nominalists. MEYRICK H. CARRÉ. London: Oxford University Press. 1946. vii + 128 pp. $2.75.

The author of this nicely printed book presents, in a leisurely but inconclusive manner, illustrative sketches of four medieval approaches to the problem of knowledge, represented by St. Augustine, Abelard, Thomas Aquinas, and William of Ockham. The chapters on Augustine and Abelard, which draw much of their content from the writings of these men, give the reader a taste of their thought on the subject of universals, sense perception, the Platonic ideas, and the metaphysics of knowledge. The chapter on Aquinas, based on a few Questions from the *Summa theologica,* covers well worn material without adding any new discoveries or insights. The final chapter, on Ockham, depends completely on secondary sources, its quotations and references being taken from the footnotes of works on Ockham, and from the brief selections translated by McKeon and Tornay. Lack of first-hand knowledge of Ockham's writings does not, however, prevent the author from forming his judgment of Ockham's nominalism as a "subversive" influence. Acknowledgment is made of the author's reliance on Gilson's historical studies, whose viewpoint is very much reflected in this book.

E. A. M.

The Sikhs in Relation to Hindus, Moslems, Christians, and Ahmadiyyas. A Study in Comparative Religion. JOHN CLARK ARCHER. Princeton, N. J.: Princeton University Press. 1946. xi + 353 pp. $3.75.

According to the author this volume is essentially a study in comparative religion in which the strength of the Sikhs is measured against that of Moslems, Hindus, Christians, and, very briefly, the Ahmadiyyas or "true Islam." The chief quality of the religion of the Sikhs seems to be its elasticity and this fits the essential concept of what a Sikh is, for he is "one who is under instruction."

The author carries the story of the five-century development of Sikhism in India from its beginning as a movement of reconciliation between Moslems and Hindus to its modern condition as a distinct religion with about six million followers, most of whom are to be found in the Punjab. The Sikhs have come through their experience fairly well, establishing an armed truce with Islam and achieving a non-committal attitude toward Hinduism. The impression is given that the Sikhs have more in common in their cultural and religious outlooks with the Hindus than with the Moslems.

With the rise of British power and the development of Westernization the Sikhs, while experiencing some discomfort, made the necessary adjustments to fit themselves into the modern world. They made some necessary rationalizations to meet the new concepts of Christianity. They accepted new standards for education. Their women were able to adapt themselves to new ways and easily took their place as equals with the men. Sikhs are Indians and Sikhs in a Hindu-controlled India could get along without too much difficulty. Most Sikhs, however, are in Moslem areas and have shown themselves able to maintain the hard nub of their identity as a separate religious cult. According to the author, "resistance has been a fairly constant mood, whether against Hindu forces which would bring about their re-absorption into Hinduism, or against British courts and policies which might endanger their communal rights, and even against measures of 'home rule' which might subject them in effect to Moslem overlordship."

The book is of current interest as it shows clearly the difficulties facing a reform or reconciliation movement which aims to smooth down the rough edges keeping Moslems and Hindus in India apart. In this instance the reconciliation movement ended by becoming a distinct cult with walls of opposition raised against the very elements which it had originally attempted to reconcile.

K. P. L.

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and Documents—Plato's Problem in the Parmenides: *Maximilian Beck.* A Note on One of James's Favorite Metaphors: *R. B. Levinson.* Bruno's Trial: *P. O. Kristeller.*

NOTES AND NEWS

The Conference on Methods in Philosophy and the Sciences will meet at the New School for Social Research, New York City, May 18, 1947. The speakers at the morning symposium on the "Aims and Assumptions of Statistical Methods" are Jacob Wolfowitz, Columbia University; Philipp Frank, Harvard University; and Paul L. Lazarsfeld, Columbia University. Ernest van den Haag, New School, Vladimir D. Kazakevich, Jefferson School of Social Science, and Karl Polanyi, Columbia University, will be the speakers at the afternoon symposium on "Postulates and Methods of Economic Theory." There will be discussion from the floor.

Our attention has been called to the fact that a number of professors of philosophy in the British and American Zones of Germany are eager to receive recent publications. Authors of philosophical articles are invited to send reprints and authors of philosophical books are urged to ask their publishers to include in their list of free copies the professors of philosophy in the chief universities of these zones, such as Erlangen, Freiburg, Göttingen, Hamburg, München, etc. The editors of this JOURNAL will be glad to supply additional information to those who wish it.

The *Bulletin of the Institute of Experimental Method* is published eight times a year. It contains articles, reviews, and abstracts concerning methodological problems and techniques of consequence to all the sciences. It brings together in one place developments in method of general interest as these developments appear in mathematics, statistics, physics, chemistry, biology, medical science, engineering, psychology, social psychology, social science, linguistics, history of science, experimental aesthetics, and philosophy of science. E. A. Singer, Jr., is Honorary Editor and C. W. Churchman is Editor-in-Chief. An Advisory Board consisting of prominent representatives of every science guides the publication.

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